

MENDELISM AND NEUROPATHIC HEREDITY.

A REPLY TO SOME OF DR. DAVID HERON'S CRITICISMS OF RECENT AMERICAN WORK.¹

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In reading Dr. Heron's pamphlet one is struck first of all by the unusual temper of the attack apparent on almost every page. The works of Davenport, Davenport and Weeks, Rosanoff and Orr, Goddard, and some others are analyzed in a fashion, or referred to, without a single feature being found in them to be worthy of anything but unreserved condemnation from the critic; and yet no other critic or reviewer has found so much to condemn in these works; on the contrary, these works have been, on the whole, very favorably received and commented on. It is natural that this circumstance in itself should arouse a suspicion of a special reason or motive underlying the attack; and perhaps in anticipation of such a suspicion our critic has felt it incumbent on himself to offer some explanations.

Thus we read: "The task of the critic is always an ungracious and unthankful one; but if Eugenics is to become a recognized branch of science with that additional sense of social responsibility among its workers that must arise when we are discussing men and not mice, then the unpleasant must be undertaken without regard to the personal feelings which strong criticism inevitably excites." (P. 4.) And further, ". . . those of us who have the highest hopes for the new science of Eugenics in the future are not a little alarmed by many of the recent contributions to the subject which threaten to place Eugenics with the older 'social science' and much of modern sociology—entirely outside the pale of true science." (P. 4.) It is evident that he would have us believe that only the interests of science, which might suffer but for his intervention, have led him to undertake the task of the critic; also that the self-imposed duty is performed with reluctance. Aside from the question of validity of the criticisms, which we shall take up later on, the very rhetoric of his style, the gusto

instilled wherever possible by harping repetitions, exclamation points, and other such devices, would indicate rather that his labor was carried on with much enthusiasm; any reluctance should surely have enabled him to confine himself more closely to the work of analysis.

Perhaps other passages in the pamphlet will throw more definite light on the question of reason or motive: "We propose to confine our criticisms to certain recent American work which has been welcomed in this country as of first-class importance, but the teaching of which we hold to be fallacious and, indeed, actually dangerous to social welfare." (P. 5.) "In this country we all know that a measure for the better control of the mentally defective has just been passed into law. No such law can touch at present those who carry this defect in a latent form, but such persons can be reached by the teaching that holds that parenthood must be looked upon as a sacred trust. The theory of Mendel has been used as an argument for the segregation of the mentally defective, and only recently we were told that to attack the application of Mendelian laws to the phenomena of feeble-mindedness was to wreck the passage of the Mental Deficiency Bill. If any argument for that bill be based on such slender considerations as the truth of Dr. Davenport's hypothesis, then the sooner the movement for the segregation of the feeble-minded is freed from such top-hamper, the less danger will there be of shipwreck." (P. 9.) "Why, we shall again be asked, does the Galton Laboratory waste its energies on destructive criticism? We shall be told, no doubt, that it is idle jealousy of the work of another laboratory. We are familiar indeed with this attitude of mind; the depreciation of well-meaning men, who do not see the gravity of the present situation—the impending danger that the new science of Eugenics will be strangled at its birth—as was the case of the once promising infant 'social science'." (P. 61.) "When we find such teaching—based on the flimsiest of theories and on the most superficial of inquiries—proclaimed in the name of Eugenics, and spoken of as 'entirely splendid work,' we feel that it is not possible to use criticism too harsh, nor words too strong in repudiation of advice which, if accepted, must mean the death of Eugenics as a science." (P. 62.)

The unfortunate position in relation to the scientific world of the English Biometrical school, to which Dr. Heron belongs, may account in some measure for the temper of the attack. They have, by reason of a pride in their own tradition, refused the guidance of the light of Mendelism, continuing to devote their time and labors to the investigation of the heredity of various traits in man as well as in animals and plants by purely statistical methods, while biologists the world over were piling up evidence of observation and experiment, continuously adding to the support of Mendel's theory. Eventually it came about that the work of the Galton Laboratory has been valued by the scientific world for the development of refined statistical methods and not as biological contribution to the subject of heredity. One may well say of the Galton Laboratory what Heine once said of his *alma mater*:

Zu Göttingen blüht die Wissenschaft,
Doch bringt sie keine Früchte.

The attitude of authorities toward the English Biometrical school has been well voiced by Bateson,⁸ as follows: "Of the so-called investigations of heredity pursued by extensions of Galton's non-analytical method and promoted by Professor Pearson and the English Biometrical school it is now scarcely necessary to speak. That such work may ultimately contribute to the development of statistical theory cannot be denied, but as applied to the problems of heredity the effort has resulted only in the concealment of that order which it was ostensibly undertaken to reveal. A preliminary acquaintance with the natural history of heredity and variation was sufficient to throw doubt on the foundations of these elaborate researches. To those who hereafter may study this episode in the history of biological science it will appear inexplicable that work so unsound in construction should have been respectfully received by the scientific world. With the discovery of segregation it became obvious that methods dispensing with individual analysis of the material are useless. The only alternatives open to the inventors of those methods were either to abandon their delusion or to deny the truth of Mendelian facts. In choosing the latter course they have certainly succeeded in delaying recognition of the value of Mendelism, but with the lapse of time the number of persons who have themselves witnessed the

phenomena has increased so much that these denials have lost their dangerous character and may be regarded as merely formal."

We have dwelt on the temper of Dr. Heron's attack and on its probable reason or motive because it may have a bearing on the validity of the criticisms: any judgment rendered under the influence of a special bias and in a state of irritation can hardly be expected to be wholly impartial. Presently we shall try to answer, as far as possible, the specific criticisms of our work; this we have undertaken to do not in the hope or even in the desire to convince Dr. Heron or others attached to the biometrical school, but rather to defend our work before a tribunal of readers more likely to be without bias.

The first point criticized is the use of the term "insanity" in the title,⁸ which use, the critic says, is very misleading, as the paper deals largely with the inheritance of the more comprehensive group of conditions under the general designation of "neuropathic constitution." An important matter is here touched on by the critic, but not one that we had overlooked. The material with which we started consisted entirely of institutional cases of certified insanity, but as we proceeded to study the genealogies in these cases we found not only other cases of certified insanity but also cases of epilepsy, hysteria, feeble-mindedness, alcoholism, and other anomalies; and in this respect the experience of every psychiatrist has been exactly like ours; the question is, then, whether only those certified as insane should be put down as affected individuals or also others presenting neuropathic anomalies but not certified as insane, and, if the latter, then what kinds and what degrees of anomaly should be regarded as justifying the counting of the individuals in question as affected. The present status of psychiatry is such that a full and exact answer to this question is hardly to be expected; and when in the study of data one is placed in a position necessitating the adoption of a definite policy the best that he can do is to guide himself partly by the prevailing judgment of other psychiatrists and partly by his own judgment and experience.

As a matter of fact certification of insanity is but an accident which may or may not come to pass in the life of a neuropathic

person; it depends on such matters as the presence or absence of anti-social manifestations, the nature of environmental conditions as tending to maintain or to disturb the mental equilibrium of the subject, the social standards of the community, etc. The policy pursued in our work is discussed in our paper as follows: "It is interesting to note that what we learn in institutional experience to recognize as insanity is a comparatively uncommon group of manifestations of the neuropathic constitution, for of our total of 437 neuropathic subjects (not counting the 21 who died in convulsions in early childhood) only 115, or 26.3 per cent, presented at any time in their lives indications for commitment to sanitariums or hospitals for the insane; moreover, it is obvious, where the facts are known in detail, that in most cases in which such indications have occurred they were in the shape of special reactions to special environmental conditions; and it seems equally obvious that our definition of the various types of neuropathic constitution must be in terms not of such special reactions, but rather of the more stable and more general underlying psychical traits and tendencies."

What is the attitude of leading psychiatrists in regard to this question?

Kraepelin* has expressed himself as follows: "The psychopathic charge of a family may reveal itself not only by the appearance of mental disorders but also by other forms of manifestation. Here belong before all those diverse slighter deviations from mental health which go to make up the borderland of insanity: nervousness, states of anxiety and compulsion, constitutional depressions, slight hysterical disorders and forms of feeble-mindedness, tics; also odd characters, peculiarities in mode of living, criminal tendencies, lack of self-control, intemperance, love of adventure, mendacity, suicide on an inner basis."

The opinion of Peterson* is very similar: "In determining the factor of heredity we must not be content with ascertaining the existence of psychoses in the ascendants, but must seek, by careful interrogation of various members of the family, for some of the hereditary equivalents, such as epilepsy, chorea, hysteria, neurasthenia, somnambulism, migraine, organic diseases of the central nervous system, criminal tendencies, eccentricities of character, drunkenness, etc., for these equivalents are interchangeable from

one generation to another, and are simply evidences of instability of the nervous system. It is the unstable nervous organization that is inherited, not a particular neurosis or psychosis, and it must be our aim in the investigation of the progenitors to discover the evidence of this."

Finally we would cite the opinion of Dr. Urquhart* as given in one of the memoirs of the Galton Laboratory itself: "But it would be a narrow view of insanity which would cause the observer to restrict his records to cases of declared failure of the integrity of mind. It is now recognized that the graver neuroses (hysteria, somnambulism and the like), that eccentricity, that a want of mental balance frequently appear among the progenitors of the insane. There is a transformation of neuroses in one generation into obvious insanity in the next. Similarly alcoholism in one generation may issue in insanity in the next, and on the other hand the most inveterate drunkards are often the immediate descendants of insane persons."

In the literature on the subject of heredity in insanity we find, in fact, but one work in which the need of taking account of neuropathic conditions other than certified insanity is ignored; the work is that of our critic, Dr. Heron,⁷ from which we quote: "The material on which the present memoir is based was most kindly provided by Dr. A. R. Urquhart, physician superintendent of the James Murray's Royal Asylum, Perth." "The Perth records consist of 331 family trees. Each gives the total number of brothers and sisters of the patient, stating the order of birth and in many cases the age of each, and classifying each as insane, neurotic, alcoholic, epileptic, eccentric or normal." "If the insane diathesis be inherited, it is much more important to know the number of relatives who have at any time been certified as insane. In the present memoir we understand by the insane members of a family those who at any time in their lives have been treated as insane." Accordingly we find in Dr. Heron's statistical analysis of his material all subjects classified as either "Insane" or "Sane" or "Not Insane," taking no account whatever of Dr. Urquhart's characterization of many cases as "neurotic, alcoholic, epileptic, or eccentric."

We do not assert and never have asserted that we possess a full and exact answer to the question of the proper delimitation of

the conception of the neuropathic constitution; we have asserted, on the contrary, that such an answer psychiatry, in its present status, does not afford; we feel, accordingly, that in classifying subjects as affected or not affected we have in all probability made some errors in both directions; but we also feel that in pursuing our policy as outlined above we have reached a far closer approximation to the truth than did Dr. Heron in his study. He has gone so far as to accuse us of having done a "disservice to knowledge"; it seems to us that we would be better justified in making such an accusation against him, had but his work received the amount of attention in the scientific world as to merit the imputation of strength implied in such an accusation.

The next point picked out by our critic is what seems to him an inconsistency on our part. We say in our paper: "In selecting cases our aim has been to exclude all those forms of insanity in the causation of which exogenous factors, such as traumata, alcoholism, and syphilis, are known to play an essential part. . . . We are not inclined to dispute the possible influence of heredity in these conditions; we have excluded them merely for the purpose of simplifying our problem by avoiding the necessity of dealing with a complicating factor in the shape of an essential exogenous cause." This in itself is not objected to by our critic; what he does object to is that having excluded alcoholic psychoses from amongst the cases selected for study, we nevertheless counted as affected cases of alcoholism occurring in ancestors or collateral relatives even if unaccompanied by obvious psychosis. What would he have us do? We could select our *cases* in any way that seemed wisest to us, but we could not choose the *ancestors or collateral relatives*; of course we could have counted alcoholics as not affected whenever we came across them, but by doing so we should have surely given him better grounds for offering a criticism than he had as it was; for, it is curious to note in view of his criticism, he himself has been led in a recent study to declare that alcoholism is very largely an expression of inborn mental defect; we will quote his own words*: "We are on fairly safe ground in asserting that the relationship between inebriety and mental defect is about .76. We have thus reached a definite measure of a relationship on which every authority on alcoholism has laid the greatest possible stress." "On the one hand, mental condition is usually re-

garded as being directly affected by alcoholic excess and on the other hand the extent of the individual's education is very largely determined by causes which are pre-alcoholic; yet we find here that there is a close relationship between the two characters, and this is strongly in favor of the view that the defective mental condition of these inebriates, like the extent of their education, is pre-alcoholic and that the alcoholism flows from a pre-existing mental defect, not the mental defect from the alcoholism." "All this lends support to the view that the mental defect of the inebriate is not an actual growth; it is born, not bred; that 'inebriety is more an incident in the life of the inebriate than the cause of his mental defect'."

We now come to a point when Dr. Heron takes up our conclusions *seriatim*, and we meet at once with a misapprehension on his part wherein he takes our statement of theoretical expectation according to Mendel's law for conclusions; we believe, indeed, that the neuropathic constitution is transmitted by heredity in Mendelian fashion but we are fully aware that our material does not show an exact correspondence between theoretical expectation and actual findings; what is more, the variable and for the most part unknown rôle played by environmental factors in the production of insanity is such that exact allowance for it can hardly ever be made, so that it may be anticipated that no collection of material will so reveal the facts as to afford a hope of finding such correspondence except by way of somewhat accidental coincidence; this, however, need not discourage us from trying to account, where it seems possible to do so, for small groups of cases which show failure of correspondence with theoretical expectation. If demonstration of the truth of Mendel's law were dependent on any material such as ours, the reviewer might well take exception to our method of analysis; or if the correspondence between theoretical expectation and actual findings, as it almost obtrudes itself upon the observer, were not at least approximately close, one might likewise take the stand of the critic and say that since so much explanation is needed, the case is unduly forced; but the fact is, it need hardly be stated, that Mendel's law is fully established on the basis of innumerable data of biological observation and experiment quite independently of our necessarily imperfect material, and that the approximate correspondence between our findings and theoretical expectation

would better justify us, in all common sense, to seek some explanation for observed exceptions rather than, on the basis of these exceptions, to reject the validity of Mendel's law in application to our case, and to assume at once that a series of traits well known to be hereditary in their essential nature is transmitted from generation to generation either in wholly irregular fashion or in accordance with some other, as yet undiscovered, law.

Thus in the case of the offspring from matings of type *a* ($RR \times RR = RR$), 64 in number, instead of all being neuropathic according to theoretical expectation, only 54 were neuropathic and 10 were normal; in examining the data pertaining to these normal cases we found that 8 of them were very young, namely, from 8 to 22 years of age, and as to these we suggested the explanation that they had not reached the age of incidence.* This explanation is objected to by our critic as follows: "We have already seen that the neuropathic constitution ranges from infantile convulsions to senile deterioration; what, then, is the age of incidence?" The answer is, our critic knows very well what the age of incidence is, for he has figured it out himself in the study already referred to from material furnished by Dr. Urquhart, giving it as " $37.9 \pm .6$ and a standard deviation of $13.6 \pm .4$." These figures of Dr. Heron's mean that if by any method it were possible to select a group of persons who were *a priori* either insane or fated to become so, the probability is that the majority of those under 24 years of age in that group would not be found to be, in fact, insane; of course it is true that subjects fated eventually to become insane may have various more or less pronounced neuropathic manifestations in childhood or at any time prior to the actual mental breakdown, but it is equally true that in the majority of such subjects the morbid tendency remains entirely latent, or at least so slight as to pass unnoticed, through the early years of life.

Similarly our critic points out that some of the subjects, the offspring of other types of matings, which have been counted by us as normal may have been young, that is, below the age of inci-

* We have made no systematic attempt to keep track of any of the subjects constituting our material; it will, however, interest the reader to learn that of the eight subjects here referred to one has, since the publication of our data, developed unmistakable evidences of mental derangement, according to information which reached us quite accidentally.

dence, and that there is a possibility that some of these were in reality fated eventually to become insane. This, of course, is not in anyone's power to deny; dealing, as we had to in our work, with subjects of all ages, the full life history was available in but a small number of cases, namely, those in which death had occurred in advanced senility; we would point out, however, that whatever error is involved here must be insignificant inasmuch as the total number of young subjects is comparatively small—only a few in the fraternities of the youngest generation, which is almost entirely the generation of our patients and their siblings, and hardly any among the sibships of the second generation, which is almost entirely that of the parents of our patients. We know of no way in which an allowance for this small error could have been made which would have provided satisfactory correction; if we had arbitrarily assumed that a certain fraction of these young subjects were fated to become insane and were therefore to be counted as affected and not normal it would have resulted, for the offspring of matings of types b and b_1 ($DR \times RR = DR + RR$), in closer approximation to theoretical expectation, and for those of types d and d_1 ($DR \times DR = DD + 2DR + RR$), in less close approximation, the net result remaining about the same as that arrived at by us without the aid of such an arbitrary assumption; Dr. Heron's suggestion to the effect that we should have excluded all subjects under the age of 38 would have resulted obviously in such over-correction of the error as but to introduce another error, of greater magnitude.

The next point of our critic's attack is on the question as to which among normal subjects should be counted as *duplex* and which as *simplex*. In a given case this question has to be decided on the basis of the presence or absence of neuropathic subjects among the ascendants, siblings, or offspring of the individual in question; for a trait which, like the neuropathic constitution, is obviously recessive, if at all transmitted in Mendelian fashion, there is no stronger evidence of a *simplex* condition than the presence among the offspring of the individual in question of affected subjects; the assumption in such cases is, of course, that the neuropathic taint carried by such a normal individual and transmitted by him to some of his offspring is handed down to him from his own ancestors, and in some cases we found, in addition to affected offspring,

other evidence in the shape of affected siblings or parents. Dr. Heron says we had committed a blunder in assuming that every normal individual with a neuropathic sibling is necessarily *simplex*; it is hard to see where he found the evidence of our having made such an assumption, for not in a single case have we counted a subject as *simplex* on the sole basis of the existence of neuropathic siblings, demanding in every case the stronger evidence of the existence of a neuropathic parent, or offspring, or both.

As to the question of classifying a normal subject as *duplex*, it must be pointed out before all that complete proof of the correctness of such classification cannot possibly be had in any case; the *duplex* condition can be *excluded*, on theoretical grounds, in the case of a normal individual who has either a neuropathic parent or a neuropathic offspring, but no other data concerning relatives can either establish or exclude it.

There is, however, no doubt of the fact that the majority of normal individuals in an average community are, theoretically *duplex* and not *simplex*, or, practically, not capable of transmitting to their offspring the neuropathic constitution; the mere fact, then, of an individual being normal turns the probability in favor of the *duplex* condition; if such an individual, known to have a neuropathic mate, has more than three children, all of whom are normal, the probability of the *duplex* condition in his case is thereby vastly increased (we say more than three children because, in particular, the offspring of our matings of the type *c* ($DD \times RR = DR$) averaged in number $3.2 \pm$ per family, not including those who died in childhood or concerning whom the data were unascertained); if in addition it is stated by informants that none of the relatives of the individual in question were known to have nervous or mental disorders of any kind, it seems reasonable to take the stand that the error which might be incurred in classifying the subject in question as *duplex* could be, for the mass of material thus treated, but very slight; and that if the only other alternative were accepted—that of classifying the subject as *simplex*—greater and at the same time purely gratuitous error would surely be incurred.

In this connection the critic takes us to task for not presenting in our charts the data concerning the ancestors and collateral relatives of the subjects classified as *duplex*. It so happens that all

but one of the matings, in which one of the mates is classified as *duplex*, have occurred either in the generation of grandparents or of great-grandparents of our patients; though we have in every such case the general testimony of our informants to the effect that none of the immediate relatives of the individual in question had, as far as they knew or had heard, any nervous or mental disorders, we made as a rule no attempt to collect data concerning each relative in particular; we felt that such data would have to be given from memory or hearsay and would be hardly of any greater value than the general negative statement, and for the few cases for which such data had been obtained we still felt that the general negative statement was of as much value as the data themselves, and were thus guided in the preparation of our material for publication. That the matter had received our attention may be judged from the following passage which we quote from our paper:

"In the actual analysis of the data collected in the course of our investigation the problem in each case was to distinguish, on the basis of the information obtained by questioning the relatives, neuropathic states from the normal state, and in the case of a neuropathic state to identify, if possible, the special variety. Such diagnosis often presents great difficulty when there is opportunity for direct observation, but when it has to be based upon observations of untrained informants related from memory the difficulty is, of course, greatly increased, and with it the chance of error. We have endeavored to reduce the amount of error from this source by interviewing personally as many as possible of the nearest relatives of the patients whose pedigrees were being investigated, and by the practice of tracing almost all the families not farther than to the generation of grandparents, for the farther back our inquiries extended the more scant and more vague was the information which we were able to obtain."

In view of the practical impossibility of absolutely establishing the fact of the *duplex* condition in any case, the fact of a chance of error in thus classifying a number of subjects must be admitted, no matter how abundant the evidence may be; it may be noted in passing that this source of error is not a discovery of Dr. Heron's at all; we had been fully aware of it and had taken it into account; we must here quote again from our paper: "On the

other hand, the fact of *duplex* inheritance was in every case based upon the absence of neuropathic manifestations in ancestors and collateral relatives, as far as known, as well as in the offspring;—but inasmuch as in scarcely any case was the family history traced farther back than the third generation it is clear that the possibility of *simplex* inheritance was in no case positively excluded; we have here, therefore, another source of error which, fortunately, is slight, and affects the least important part of our material, namely, the cases of matings from which no neuropathic offspring have resulted.”

Before passing on to the next and last criticism we would point out what can only be considered either a wilful misstatement or another misapprehension on our critic's part. He asserts repeatedly that the subjects whom we have tentatively classified as *duplex* are said by us to be “normal and of pure normal ancestry”; this is simply not true; the truth is that not a single subject investigated by us has been described either in the paper criticized by Dr. Heron or in any other paper published by us as being “normal and of pure normal ancestry”; on the contrary, our attitude is and has always been that it would be impossible in practice to say that truthfully of any subject; the expression has but a theoretical value and is used by us only in the statement of the theoretical expectation in accordance with Mendel's law. We have already had occasion above to refer to his mistaking our statement of theoretical expectation for conclusions; this is evidently a part of his general method; he might as well have asked how it was that we had “drawn the conclusion” that “Both parents being normal and of pure normal ancestry, all the children will be normal and not capable of transmitting the neuropathic make-up to their progeny,” without having in our material a single instance of such a mating showing such results: it isn't a conclusion at all and is not offered as such; it is a part of the statement of theoretical expectation according to Mendel's law, which is made by us in the belief that we have gathered convincing evidence to show that that law holds for the case of neuropathic heredity.

In approaching the end of the section devoted to the criticism of our paper Dr. Heron, apparently carried on by sheer inertia, is glibly “rejecting” without even stopping to say just what it is he

is "rejecting" and why; finally, however, he does offer one more specific criticism; in his parting shot there is so much that is illustrative of his temper and method that we feel impelled to quote his words in full, simply trusting that the gentle and patient reader will forgive us for so doing: "It is unnecessary to follow the authors in their discussion of degrees of recessiveness or of equivalent defect, but it may be noted that the last result, that about 30 per cent of the general population, without being actually neuropathic, carry the neuropathic taint from their ancestors and are capable under certain conditions of transmitting the neuropathic make-up to their progeny, must also be rejected. Apart from any other blunders, the authors have forgotten that when a simplex individual mates with a neuropathic or another simplex, it by no means follows that at least one of the children will be neuropathic; in the latter case three-fourths of the families of one, nine-sixteenths of the families of two, etc., will have *only* normal offspring. Even an elementary knowledge of Mendelian theory would have been sufficient to enable the authors to avoid such an obvious pitfall."

As regards the last remark we submit that the point at issue has nothing to do with Mendelian theory and that therefore one's knowledge of Mendelian theory is not shown in the way he deals with that point; it has to do rather with theory of probability. He declares we "have forgotten" that among the offsprings of the matings in question there will not necessarily be in every family at least one affected; now, whatever might be said of Dr. Heron as a critic, we are here impelled to point out that as a mind-reader he is an utter failure; he must surely grant that on matters of purely subjective fact, such as what we have tacitly assumed or what we have forgotten, we possess better information than he; we have already told him that what he said we had assumed we had, in fact, not assumed; we can now tell him that what he says we have forgotten, we have, in fact, not forgotten. The reason for our taking into account, in the above connection, *only* the families in which there was at least one affected subject is that *only* such families contain, in the shape of the affected subject, justification for counting both mates as having the neuropathic taint; as regards other families in which none of the offspring were affected we admit the possibility that both mates may have

been *simplex*, but there is no *proof* of it in any case. If it should be argued that, in view of this possibility, our figure is probably an underestimate, we would say that that, too, is quite likely; the trouble is, however, that no data are available that would enable one to determine to what extent our figure is an underestimate and, therefore, how much must be added for correction. However, our critic might have realized that our figure is offered not as an exact estimate of the prevalence of the neuropathic taint; who in the world could make an exact estimate? What we meant was to draw attention to the evidence contained in our material which startled us and which, in general, is hardly suspected, showing that a very large proportion of the general population is capable under certain conditions of producing neuropathic offspring; for the present, at least, importance attaches not to the question whether the exact proportion is 25, or 30, or 35 per cent, but rather to the fact that it is not some fraction of one per cent, or two, or three per cent. Had our critic, indeed, not gone out on the war path determined to find fault, he might have noted that we said "Our data seem to show that no less than 31.6 per cent of the general population carry the neuropathic taint." Anyone might know that "our data seem to show" does not mean "all data would, or ever shall, show," and that "no less than 31.6 per cent" does not mean "no more than 31.6 per cent." If later, in the conclusions, we say, in reference to the same data, that "It is estimated that about 30 per cent (deducting 1.6 per cent for those actually neuropathic) of the general population, without being actually neuropathic, carry the neuropathic taint," etc., it would seem clear that we do not mean "It is fully established that exactly 30 per cent," etc.

We fear we have already fatigued the reader with this sorry business of refuting a manifestly unfair and incompetent criticism; our plea is, however, that our critic having made the attack, it remained for us either to say nothing, and thus possibly allow some to think that we had no answer to make, or to answer; we have thought enough of our work to consider that it merited a defence; others, whom we consulted, have thought the same, and we have accordingly chosen the latter course. Such being the situ-

ation we feel that our answer, to be full, must take into account not only the actual criticisms, but also every other phase of the attack. Therefore, we still have to refer to a passage in the pamphlet which reveals a feature of method not quite so apparent elsewhere, namely, *insincerity*. The passage is directed not only at our own work, but at the whole group of American researches; the critic declares, "The authors have in our opinion done a disservice to knowledge, struck a blow at careful Mendelian research," etc. Who is it that pretends to resent "a blow at careful Mendelian research"? It is a representative of a school which has always disputed the validity of Mendelism and the scientific standing of which in the field of heredity has heard its death-knell sounded as a result of "careful Mendelian research." The data of human heredity, particularly in the domain of psychic traits, can never compare with the best data of experimental biology; if one is led to deal with them it is owing to their importance and not to any notion that they are possessed of the highest scientific value; fortunately, in dealing with them, guidance having been made available by biological science, the problem before the investigator is not to discover or establish laws but to apply them, and material which may be unfit for the former purpose may serve very well for the latter.

To summarize, our critic, in spite of his evident determination to disprove the value of our work, has not succeeded in finding sources of error of which we were not ourselves cognizant and which we had not ourselves frankly discussed in our paper; neither he, nor we, nor others have as yet succeeded in suggesting any way of eliminating such sources of error. The main question that has relevancy as to the trustworthiness of our material is whether subjects counted by us as affected or not affected have been thus counted correctly; the question is purely one belonging to the domain of clinical psychiatry; our policy in regard to this question has been that which bears the approval of present-day psychiatric science and which is based on the universal experience of clinicians; that some mistakes have not been avoided after all, that some cases counted as affected have been wrongly so counted, and that others counted as not affected have also been wrongly

so counted, is undoubtedly true, and we have said so in our paper: "On the whole no pretension is made here of total elimination of error; but we believe that whatever errors remain they are not sufficient to invalidate the material as a basis for our study." This belief rests on the conviction that the errors are slight and, being in both directions, balance themselves to some extent. The burden of proof is upon the critic who, though a layman, assumes an attitude, in relation to a psychiatric issue, which is in opposition to a view universally held by psychiatrists; and if he, furthermore, attempts to disqualify, on the basis of his attitude, work which in his opinion contains too large a margin of error, he must in addition take the burden of furnishing an acceptable measure of the error before his criticisms can be rendered valid; this our critic has not done.

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